LETTERS TO THE EDITOR.

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Fertilising Effect of Soil Sterilisation.

WITH further reference to the work of Messrs. Russell and Hutchinson on soil sterilisation (*Journal of Agricultural Science*, October, 1909), it may be interesting to record some information of which I have recently become possessed.

Some of the large growers of cucumbers, tomatoes, &c., under glass for the London market have for some little time adopted the plan of injecting jets of steam into their soil before planting, not with any view of increasing its fertility, but with the view of destroying slugs, insects, &c. In the experience of some growers the productivity of the soil after steaming has become so greatly increased that, if anything like the usual quantity of stable manure is mixed with the soil, the plants grow with such rank luxuriance as to spoil their bearing capacity, exhibiting all the symptoms that would be expected as the result of a heavy overdose of nitrogen.

This experience has been communicated to me by growers who were previously unaware of the Rothamsted work. At the moment they were feeling in somewhat of a dilemma: if they did not steam the soil they suffered from insect pests; if they did steam it they were obliged to curtail the supply of stable manure, at the expense of lowering the subsequent soil temperature, which is normally maintained at a high level by the fermentation of the manure. No doubt means may be found of adjusting the various conditions satisfactorily, but meantime the observation appears to afford striking independent confirmation on a practical scale of the indirect fertilising effect of partial sterilisation in killing off the phagocytes or protozoa which normally keep down the numbers of those bacteria the task of which is to turn organic nitrogen into plant food. BERNARD DYER.

17 Great Tower Street, London, E.C., March 15.

Certain Reactions of Albino Hair.

IN a note in the *Journal of Physiology* (vol. xxxviii.) on the chemical nature of albinism, Mr. Mudge describes some interesting observations which he made upon rats' skins. Starting with the presumption, based upon the work of Miss Durham and Cuénot, that an albino carries a chromogen and lacks the ferment necessary to produce pigment from it, and supposing that fermentation is a process of oxidation or reduction, Mr. Mudge argued that it might be possible to produce pigment artificially by means of an oxidising or reducing agent. He found by experiment that immersion of albino rat skins in a solution of 10 per cent. formalin and 70 per cent. alcohol in equal volumes resulted in a "vivid yellow colour" in the hairs; he further states that these coats, when washed in water and immersed in H_2O_2 (20 vols.), become changed in colour from vivid yellow to a "brownish tint" in about twenty-four hours. He adduces arguments to show that the coloration is due to the presence of a specific body in the hairs diffused through the keratin, and not to mere reaction between the keratin and the formalin.

I have repeated these experiments with various skins. In the case of the single albino rat skin which I used, the vivid yellow was obtained immediately on immersion in the formalin mixture. The change to brown in H_2O_2 was not obtained, but complete decoloration resulted from immersion in this reagent. Prolonged immersion in the formalin mixture also produced almost complete decoloration.

With guinea-pig albinos carrying, respectively, black, chocolate, and red, negative results were obtained, as they were also with a single mouse skin.

What struck me as particularly interesting in connection with the yellow colour produced by the formalin mixture in the coat of the albino rat is the fact that it is a peculiar canary-yellow, which I remember to have seen elsewhere among mammals only in members of the stoat family when the winter whitening is incomplete. A piece of pale yellow stoat fur acquired a much more intense yellow colour as a result of twenty-four hours' immersion in the formalin mixture; a similar piece was decolorised by H_2O_2. There can thus be little doubt that the yellow body produced artificially in the fur of the albino rat is a substance similar to the yellow pigment of the stoat's winter coat, and therefore probably represents a stage in the reduction of the pigment to the condition in which it exists in the white hairs.

Miss Durham tells me that it is a well-known fact that albino rats do not remain pure white if they are exposed to the action of light. Just as darkness is necessary for the production of a pure white coat in the rat, so a certain degree of cold seems necessary in the case of the stoat tribe, though in their case a change to a warmer climate does not at first prevent the usual colour-change in winter. Thus Eric Parker, in "The Book of the Zoo," points out, concerning a certain foreign pine-marten, that "the first winter he spent in the Garden his fur turned almost white to match the snows he would naturally have expected. The last two winters it remained brown, though it has lightened considerably towards yellow." This repetition of a periodic act without the usual stimulus recalls certain phenomena in plants, which Mr. F. Darwin attributes to memory. IGERNA B. J. SOLLAS.

Nitrogen-fixing Bacteria and Non-leguminous Plants.

MAY I be allowed to direct attention to two errors in Mr. Hall's letter in NATURE of December 23, 1909?

Mr. Hall states that "Pseudomonas and Azotobacter together (1·24) are less effective than when grown separately (0·91 + 0·56)." This comparison is incorrect. The fixation of free nitrogen by bacteria is estimated in terms of milligrams of nitrogen *per unit of carbohydrate* in the culture solution. Pseudomonas and Azotobacter together give 1·24 N for *one* unit of carbohydrate. Pseudomonas and Azotobacter grown separately give 1·47 N for *two* units of carbohydrate, hence the correct comparison is:—

	Per unit
Pseudomonas and Azotobacter together	$= 1 \cdot 240$ N
Pseudomonas and Azotobacter separately	$= \frac{1 \cdot 47}{2} = 0 \cdot 735$ N.

Hence my conclusion that Pseudomonas and Azotobacter together are *more* effective than when grown separately is, I think, justified.

The second error has reference to a mean experimental error of ± 10 per cent. Mr. Hall writes:—"By an error which the context rendered sufficiently obvious, I wrote "oats" instead of barley when dealing with Prof. Bottomley's first-quoted experiment with soil." May I point out that oats were the only plants mentioned in the "first-quoted experiment with soil"? Even if the increase of barley (13·6 per cent.) be taken, one fails to see how it is "sufficiently obvious" that a mean error of ± 10 per cent. *more* than covers an increase (the lowest of the results quoted) of 13·6 per cent. W. B. BOTTOMLEY.

King's College, Strand, W.C., February 16.

A Sample of Spurious Correlation.

THOUGH regretfully unable to do justice to the mathematical reasoning of Dr. G. T. Walker in NATURE of January 6, I may, perhaps, be allowed to say that it is of the essence of the method that those dots (each expressing a comparison of two sums of thirty items) tend to arrangement in a straight band, or strip, with fairly defined borders. It is expected that future dots will generally come within those limits; but to affirm this in a given case, to say, *e.g.*, that the next dot will not be below a certain level, is it not, necessarily, to say something quite definite as to the character of the coming season, as that its rainfall, frost days, or other feature considered, will not be below a certain numerical value? If the one statement is warranted, so (by the nature of the case) is the other. Thus the essential point seems to me to be whether the past distribution of those dots affords a reasonable clue to their future distribution, and I do not see that my critic throws doubt on this.

I think (with all deference) that anyone who will give the method a full trial will find it distinctly helpful in a number of cases (I do not say in all).

ALEX. B. MACDOWALL.

8 Marine Crescent, Folkestone, January 14.

IN my former letter in NATURE of January 6 I attempted to prove that the arrangements of dots in a band would occur even if the numbers of which the sums were taken were entirely independent of one another, in which case a forecast regarding one of the numbers could not possibly be made from knowledge of the remainder. A forecast could only be made if it were shown that the width of the band were smaller than would be expected on the hypothesis of pure chance, and this vital point has received no consideration.

The situation may be made clearer by reference to the original letter in NATURE of September 16, 1909. The essence of the method is that, if we were forecasting for 1910, the dot the two rectangular coordinates of which are the sums of data for thirty years up to 1909 and 1910, respectively, will lie near to a line through the origin at an angle of 45° with the axes. Thus the sum of the data from 1880 to 1909 will be *nearly* equal to the sum of the data from 1881 to 1910, or the data for 1880 and 1910 will be *nearly* equal. If the *nearness* to equality has any value at all for forecasting, this is equivalent to asserting that the data in question tend to be repeated after thirty years, or have a thirty years' period; but as the same result could be reached if 25 or 35, or any other comparable number, had been substituted for 30, it will be seen that the reasoning cannot be free from error.

That the nearness to equality is inadequate is clear from the diagram in the original letter. The edges of the band there intercept a length representing about thirty-six days along any vertical ordinate. Hence all that can be inferred in forecasting for 1910 is that the number of hot days will probably not differ by more than ± 18 from the number of hot days in 1880; and as the average number of hot days in a year is stated as fifteen, it appears that a forecast so entirely vague could be made without any analysis whatever.

GILBERT T. WALKER.

Kodaikanal, February 21.

SOME SCIENTIFIC CENTRES.

No. XV.—THE MOUNT WILSON SOLAR OBSERVATORY OF THE CARNEGIE INSTITUTION OF WASHINGTON.

MOUNT WILSON rises 6000 feet, almost abruptly, from the plain in which lie the twin cities of Los Angeles and Pasadena. From the mountain top these cities appear at night as glittering star clusters; by day they are seen through a haze of dust which the ascent of the mountain has put below our feet. Beyond is the vast Pacific; above our heads the glorious sky of California; around us the buildings of perhaps the best equipped observatory in the world.

These words are written by anticipation. Prof. Hale has invited the International Union for Solar Research to hold its next meeting on Mount Wilson on August 29, 1910, and astronomers and physicists from all parts of the world are eagerly looking forward to the occasion. The present writer is not, however, altogether a stranger to the scene; he was on Mount Wilson in 1904; but at that time the observatory was in its infancy. It had not even been decided on what scale it was to be designed. Prof. Hale had realised the magnificent opportunities offered by the climate and site, and he had made urgent application to the Carnegie Institution for funds adequate to deal with the serious difficulties to be overcome; but he had also resolved that, if his application was not granted, there should still be a solar observatory on Mount Wilson, for which he would himself provide the funds, so that he had already commenced building operations. Nevertheless, the utmost provision which he and his courageous wife could afford to make would naturally fall far short of what was needed for a suitable observatory, and he was therefore anxiously awaiting the answer of the Carnegie Trustees. Fortunately for astronomy, it was favourable; and since it was received one marvel has followed another in rapid succession. The visitors will be drawn to Mount Wilson as to the main focus of astronomical enterprise and success at the present moment.

The first of the principal instruments to be completed was the great horizontal Snow telescope, originally constructed at the Yerkes Observatory, with the aid of funds given by Miss Snow, of Chicago. The concave mirror, of 24 inches aperture and 60 feet focus, is fed by a cœlostat with plane mirrors of 30 inches and 24 inches, the beam of light being sheltered by a house specially designed to guard against temperature effects. To this telescope can be attached a spectrograph of 18-feet focus, or a 5-foot spectroheliograph. The heavy parts of the apparatus are mounted on massive stone piers, built with great labour, since it was found that the stone in the neighbourhood was unsuitable, and that materials had to be brought up from a lower altitude by mules.

But, successful as this powerful instrument has been, it has also served to point the way to possible improvements. Experience of its working suggested that a *vertical* telescope might be in various ways better than a horizontal one; and accordingly a "tower" telescope was constructed, with the cœlostat mounted on a tower 60 feet high, built as a skeleton framework. This experiment was so successful that a more ambitious one was at once projected, and a tower 150 feet high is under construction. As wind pressure will be much more serious on this new structure, Prof. Hale has adopted the ingenious device of building an outer tower for protection, surrounding every bar of the inner tower by a tube of the outer. The lower parts of these tower telescopes are contained in wells sunk many feet into the ground.

Thirdly, there is the beautiful 5-foot reflector, made by Prof. G. W. Ritchey, who has already proved his skill in such work. The mirror was made at the Yerkes Observatory some years ago, but has had to wait until a mounting could be provided on Mount Wilson; and, indeed, there was a still earlier provision to be made; the track up the greater part of Mount Wilson was originally only 3 feet wide—a mere ledge in a precipitous descent—and up this narrow track the materials and instruments were carried, at first on mule back, later in a specially designed carriage, with steering fore and aft, and drawn by a mule. But the 5-foot mirror and its mounting could not be taken up in this way, and it was necessary to widen the track to 5 feet throughout its whole length. This was not accomplished without serious delays, owing to severe storms, which sometimes destroyed weeks of labour; but it was finally completed, the instrument was taken up and mounted, and at the meeting of the Royal Astronomical Society on December 10, 1909, were shown some photographs of Mars taken with this great telescope which far surpassed anything of the kind yet seen, and for which the president was requested to convey a special vote of thanks.

A still larger telescope, with a mirror 100 inches in diameter, is to be attempted on Mount Wilson, but is not yet within sight of completion. Round the existing three great instruments are grouped a number of other buildings; first and foremost a physical laboratory, so indispensable now in astrophysical work; also an astrophysical museum, and a variometer house; and then such necessary accompaniments as a power-house, a pump-house, storage-houses, and dwellings. The establishment is not adapted for ladies and children, and the chief residence is called the "Monastery." Distressing news reached us recently that the "Monastery" had been burnt down, owing to the carelessness of a temporary servant. Fortunately it contained no original photographs or records, and most of the books had been